A Study of the Balance.

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(Communicated by C. V. Boys, F.R.S. Received December 15, 1921.)

This research was not undertaken with the deliberate aim of exceeding the precision of mass-determinations as carried out in the great institutions specially devoted to this subject. I should have considered it both presumptuous and hopeless to attempt such a feat, and should probably have abandoned my studies at an early stage if it had not fortunately happened that the decisive part of my work had been completed before I came across comparable results obtained elsewhere. It was only then that I realised that the methods evolved had led to a very considerable diminution of the residual errors of weighings, in spite of the inexpensive instrument employed and of the primitive conditions under which the observations were made.

A projected series of experiments called for weighings of small masses, up to perhaps 50 grm., within 0·1 mgrm. As I did not possess a balance, I bought the least expensive type of analytical balance, one by Sartorius, to carry 200 grammes in each pan, with a triangular phosphor-bronze beam, 14 cm. long, with agate knife-edges and plane bearings, with a complete arrestment for beam, suspensions, and pans, and with a rider-scale divided to read directly to 0·2 mgrm. by the usual centigramme-rider. The cost at the time (1905) was £6. The balance was described in the catalogue as "sensitive to 0·2 mgrm.," but I trusted that the required accuracy within 0·1 mgrm. would be obtainable by using the more refined weighing methods of the physical laboratory.

Naturally, the first work undertaken consisted in determining the errors of the set of analytical weights to be used with the balance. The plan described in Kohlrausch's 'Practical Physics' was adopted, and the simple Gaussian method of exchange was used so as to eliminate the index error and the inequality of the arms of the balance. The observations were reduced within 0·01 mgrm. with occasional retention of 0·005 mgrm. This first calibration had been begun within one hour of the time when the balance had been taken to pieces in the room in which it was stored and re-erected in another room. On consideration of the fundamental nature of the work, this procedure appeared to cast grave doubts upon the reliability of the results. The eight comparisons of the brass weights were therefore repeated a week later, the balance and weights having been given three hours to settle down to the temperature of the surroundings before observations were commenced. The method of double exchange was used (first, A in left pan; second, B in left pan; third, A again in left pan), in order to eliminate also changes of the zero-point so far as they are proportional to elapsed time. When these observations were reduced the surprising and entirely unexpected result emerged that the greatest disagreement with those of the previous calibration was only 0·01 mgrm. (on the actual weighings, not on the deduced errors of individual pieces), and that the probable error of one weighing—all treated as of equal reliability—was of the order of 0·004 mgrm., or one-fiftieth of the possible discrepancy suggested by the makers of the balance.

It was this high precision of the first results obtained from the balance which caused me to postpone the projected experiments, and to make the balance itself the subject of further study. It appeared probable that still better results might be obtained by improvements in the adjustments and by more care in the actual observations. A long series of experimental weighings was thus begun, but for 1½ years no progress whatever was made in the accuracy of the results. The probable error derived from any reasonably extensive series of weighings remained with singular obstinacy in the neighbourhood of the value 0·004 mgrm. originally found, in spite of the fact that a succession of refinements applied to the instrument and to the methods of observation and reduction were without exception of a kind which must greatly diminish the effects of the sources of error at which they were aimed. This led to a growing conviction that there must be present some source of error of greatly preponderating magnitude which had escaped detection. This conviction was strengthened by the fact that occasionally the results of a series of weighings were decidedly worse than usual, and led to a note in the journal: "Balance evidently in bad condition," the observer being included in the indictment in one or two instances.

Eventually a bad case of this kind led to the discovery of the elusive preponderating source of error. The knife-edges, being made of material of limited strength and rigidity, necessarily do not terminate in an absolutely sharp edge, but have a finite radius of curvature of the order of 0·01 mm. As a consequence, the effective length of the lever-arm at which the load is applied would change if the plane resting on a terminal knife-edge were rigidly attached to the pan, and if the weight in the latter were put into varying positions in successive exchanges, for the planes would then become tilted, and the contact line would shift (fig. 1). To avoid this well-known source of error all balances have an "intermediate joint" between the suspension and the pan, so that the latter can swing independently of the former. In the Sartorius balances the joint primarily relied upon for this purpose takes the form of two round pins at the upper end of the stirrup from which the pan is suspended, these pins resting in larger holes in the lower ends of the fitting attached to the agate plane. If the fulcrum thus provided were a definite and frictionless one it would ensure that the load itself would force the agate plane always into the same orientation with reference to the horizon. But as the pins are over 1 mm. in diameter and rest in holes of about 2 mm. diameter, they may take any position within the sliding angle of friction of 5°, or more, from the really lowest point of the hole, and so become themselves a cause of tilted end-planes instead of preventing such tiltings.

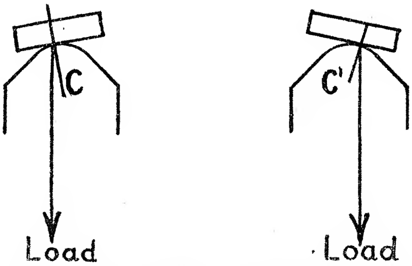

Fig. 1.—Tilted end-planes cause change of lever arms.

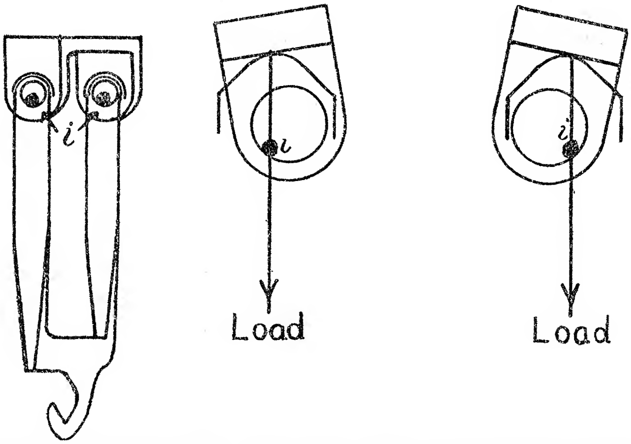

Fig. 2.—Sartorius intermediate joint allowing end-planes to tilt.

This defective intermediate joint was definitely identified as the source of the large errors, the aggravation occurring when the stirrup happened to rest with friction against one of the cheeks of the upper fitting. The cure applied consisted in rendering the original intermediate joint immovable by tightly screwing its two parts together, and in relying on the hook at the lower end of the stirrup as a more definite and efficient intermediate joint.

As soon as this change had been made, the probable error of the observations for a calibration of a set of decigramme weights went down to 0·0013 mgrm., or to one-third of the previous value. Each complete weighing in this set consisted of four partial weighings—

(1) A in left pan,
(2) B in left pan,
(3) B again in left pan,
(4) A in left pan,

and the sensitivity was determined at frequent intervals by a fifth observation with a suitable added load produced by a shift of the rider. The latter weighed 1 mgrm., and was not shifted during the four partial weighings.

The discussion of the residuals of this series immediately revealed another but much smaller systematic error. It was found that the sign of the residual error of any one complete observation depended very decidedly on the sequence of the deflections of the pointer in the partial weighings. If at the first partial weighing the deflection was larger than at the second, the difference A–B tended to be found too small, and conversely in the reverse case. This was tentatively attributed to the imperfect elasticity of the knife-edges and of their bearings. These must be—on account of the extremely short radius of curvature—loaded almost to the limit of elasticity. Hence the deformation ("flattening") of the edge produced by the load in any one observation would disappear only partially on arrestment, the residue of deformation remaining for a considerable period. The consequence must be that the balance retains during that period a bias in favour of the previous position of rest and gives a false reading at the succeeding observation. This led to an effort to devise a sequence of partial observations which would defeat this bias of the balance. An obvious method would be to take advantage of the light rider and its finely divided scale, and to find by a series of approximations such an addition by ridershift to the lighter mass that eventually the deflections in successive exchanges would be practically identical. That method, however, would call for several wasted observations. The method really adopted, and adhered to ever since, was a simple modification of the well-known series of four partial weighings already referred to, the object being to attach the "bias" twice in one sense and twice in the opposite sense to the partial results, and so to cause its complete cancellation in the mean of all four.

Assuming that the residual difference of the two masses A and B is so small (or has been so adjusted) as to keep the variation of deflection on exchange within practicable limits, a wasted observation—not recorded—is first made with B in the left pan to adapt the knife-edges to the load and to establish the corresponding bias. Then follow the four real observations :—

(1) A in left pan, deflection D_1, which is at once determined from the pointer readings.

(2) B in left pan, deflection D_2, also determined immediately after the readings of the pointer. D_2 will of course closely correspond to the deflection in the preliminary observation.

(3) B still in left pan, but with such an addition by ridershift, easily calculated from the known sensitivity, as will cause the deflection to become as nearly as possible equal to D_1.

(4) A in left pan, but with such a ridershift as will cause the nearest possible approach to deflection D_2.

By observation (1) A–B is obtained, afflicted with the bias produced by a change from previous deflection D_2 to new deflection D_1. By observation (3) B–A is obtained, afflicted by the same bias as observation (1), but attached to the reversed position of the two masses. Hence the difference (1)—(3) will give 2 (A–B), but the bias cancels out. The same argument applies to the combination of observations (2) and (4). The set of four observations simultaneously cancels any progressive change in the balance during the time required for the complete set, which in my case was about 12 minutes.

The preliminary wasted observation was usually omitted, as it was found that the small systematic error (less than 0·001 mgrm.) aimed at was sufficiently removed by making only the four actually used partial weighings.

The first trial of this method on the same set of decigramme-weights gave a probable error of one complete weighing of 0·0008 mgrm., or only one-fifth of the original error and less than two-thirds of the one obtained without the refinement of the "alternating position of rest."

Up to this stage of the experiments the balance had been used in the usual laboratory way, the pointer being read with the aid of a fixed lens of 5 inches focal length, and the sliding door of the balance being sufficiently opened during the exchanges to allow of the use of a long forceps. The latter, however, was of such length that the warm hand remained entirely outside the balance case. In spite of this precaution air-currents induced by the use of the sliding door appeared to be largely responsible for the residual errors. Decidedly crude arrangements were therefore made, which allowed the balance case to remain closed all the time, the loads being handled by the original long forceps attached to the case by a washleather conical tube. A considerable number of calibrations of sets of weights was then carried out, and gave probable errors of one complete weighing ranging from 0·0005 down to 0·0003 mgrm.

It may be briefly stated that practically all the experiments were carried out on sets of weights, mostly fractions of a gramme, of the pieces 1, 2, 3, 4 and 5. Comparison of these among themselves and with 10, the unit of the next decade, gives twelve different combinations: $1 + 2 = 3$, $1 + 3 = 4$, $1 + 4 = 5$, $2 + 3 = 5$, $2 + 3 = 1 + 4$, $2 + 4 = 1 + 5$, $3 + 4 = 2 + 5$, $1 + 2 + 3 + 4 = 10$, $1 + 4 + 5 = 10$, $2 + 3 + 5 = 10$, $2 + 4 + 5 = 10 + 1$, $3 + 4 + 5 = 10 + 2$. If the first two comparisons are repeated a second time, fourteen observations result, the normal equations of which are direct solutions for the five corrections as the mean of ten observations in the case of the pieces 1, 2, 3 and 4, and as the mean of eight observations in the case of 5. Owing to the large number of observations a good determination of the probable error is secured; the great variety of combinations moreover renders it unlikely that any systematic error should remain undetected. Mere repetitions of the same comparison, done always in the same way, are apt to give quite fallacious ideas as to the accuracy attained.

About the time when my work had reached the stage described, a volume of "Travaux et Mémoires" of the International Bureau at Breteuil appeared, in which results of comparable calibrations of weights are given. The probable error for small weights up to 1 grm. ranges from about 0·0015 to 0·003 mgrm. for one complete weighing; it is, therefore, on an average, five times as large as the one I reached with my comparatively heavy balance. The load on the central knife-edge of my balance was about 110 grm. when very light aluminium pans were employed for comparisons of small weights. The Breteuil weighings were made on a balance specially constructed for loads not exceeding 1 grm., and the whole of the moving parts probably weighed less then 10 grm. I have only come across one authenticated set of weighings, also for fractions of a gramme, which gives a much lower probable error than that secured at Breteuil. This set is mentioned in a note in "Zeitschrift für Instrumentenkunde," from the Berlin Institute of Weights and Measures, and leads to a probable error of 0·0008 mgrm., or twice that in my final observations. The balance used was of the same type as that employed at Breteuil. Both the Breteuil and the Berlin observations were made telescopically from a considerable distance.

As it seemed desirable to confirm the conclusions at which I had arrived, I bought another very small Sartorius balance with a beam only 8 cm. long, "to carry 10 grm. in each pan, and sensitive to 0·1 mgrm." This showed the same inconstancy as the 200 grm. balance until the intermediate joint of the suspensions was rendered immovable. It then yielded results by the method of alternating deflections of even higher precisions than the heavier balance, probable errors down to a little over 0·0001 mgrm. being repeatedly recorded. An even more satisfactory confirmation of my conclusions was obtained from a rough "bullion-balance" for loads up to 1 kilo. This was fitted with plane bearings (the original ones were roof-shaped), and with suspensions designed to minimise the effects of the finite radius of the terminal knife-edges, also with a crude but reasonably draughtproof case. It thus became an extremely satisfactory precision balance on which heavy weights can be compared well within 0·01 mgrm.

Shortly before the outbreak of the Great War, Sir Richard Glazebrook kindly consented to have some experiments made with my original balance in the constant-temperature rooms at the N.P.L., the balance having in the meantime been fitted with mirror and telescope, so as to allow of its use on a distant scale with a greatly increased deviation per milligramme. The suspensions had also been further modified. It was found that masses up to 100 grm. could be compared within about 0·001 mgrm.; I had never succeeded in doing this at my house, on account of rapid changes of temperature, vibration from traffic, and residual defects in the original suspensions. These experiments at Teddington were interrupted by the War and have not been resumed; but I understand that my balance is largely used for weighings of great accuracy.

A number of refinements in both the balance and in the method of observation have not been mentioned in the foregoing account of my experiments; these additional desiderata are included in the following concise account of the essential features of a balance of highest precision and of the proper method of using it. A reliable judgment in these respects can only be based upon careful *quantitative* estimates of the known sources of error. The accuracy reached and occasionally exceeded in my experiments, amounted to 7×10^{-9} of the load on the terminal knife-edges (*i.e.*, 0·001 mgrm. in 140 grm.), and to $2{\cdot}5 \times 10^{-9}$ (0·001 mgrm. in 400 grm.) of the load on the central knife-edge. To render possible this degree of precision, the length of the lever-arm (70 mm. in my case) must have been "definite" within the same minute fraction; but it might, and undoubtedly did, change proportionately to time by a large multiple of that fraction even in the 12 minutes required for one complete weighing. The important point to be noted is, however, that the supporting line of the central knife-edge must, at any given moment, be definite within $70 \times 2{\cdot}5 \times 10^{-9}$, or $1{\cdot}8 \times 10^{-7}$ mm. (1·8 Ångström units !), and the contact-line of the terminal knife-edges must be definite within 5×10^{-7} mm. These are the quantities on which reliable estimates may be based. Their minuteness renders plane bearings absolutely indispensable, for, with cylindrically curved or roof-shaped bearings, a very slight lateral shift would change the contact-line by many times the permissible variation in length of arms.

The Beam.

Many elaborations found in the design of the beam are of little if any value. There should be as few joints and screws as possible, for every one of these may under changing temperature become a source of sudden minute shifts of the centre of gravity. By far the most important point is the fixing of the knife-edges. These are usually simply driven into prepared seatings In the Sartorius balances they are neatly fitted in this way, but, in addition, a cement (apparently shellac) is used to fill the interstices. My kilogramme balance originally had the steel knife-edges merely tightly driven into the aluminium beam. Repeatedly there were sudden shifts of the zero point. These were tentatively attributed to the release of strains between the knife-edges and their seatings, caused by the difference of thermal expansion. This assumption appeared to be correct, for, on a careful and sustained treatment with shellac-cement, this trouble vanished, never to return. Similar disturbances may be traced to a slightly loose or badly fitted screw taking a new position with a change of temperature. The short beam, first introduced by Bunge, has a great advantage over the formerly preferred long beam, because it is more rigid, gives a higher sensitivity at any given time of vibration, and is also less affected by inequalities of temperature.

The Knife-edges and their Bearings.

The terminal knife-edges require means for the well-known adjustments to establish parallelism with the central knife-edges, equality of the lever-arms and unchanging sensitivity with varying loads. The Sartorius arrangements for this purpose are both convenient and reliable. By far the most important one of these adjustments is that for parallelism of the projections of the three knife-edges upon a horizontal plane, for any departure from this parallelism means that the lever-arm changes according to the point of the terminal knife-edge below which the centre of gravity of the load is situated. It is difficult, if not impossible, to secure this parallelism for any length of time or for varying loads within less than 20 seconds of arc or 0·0001 radian. As the lever-arm must be definite within 5×10^{-7} mm., this defect in parallelism makes it necessary that the load shall always be applied within 0·005 mm. of a fixed point in each terminal knife-edge. Balances therefore always have in their suspensions some kind of "cross-joint" or fulcrum with an axis at right angles to the knife-edge which is supposed to assure this adjustment of the centre of gravity of the load automatically, but rarely does so with sufficient precision. The second source of error at the terminal knife-edges arises from their finite radius of curvature of about 0·01 mm. which causes a change in the length of the lever-arms if the planes bearing on the knife-edges are tilted. The permissible tilt is found by dividing the allowable variation of the lever-arm of 5×10^{-7} mm. by the estimated radius of curvature of 0·01 mm. and is therefore 0·00005 radian or only 10 seconds of arc. None of the usual "intermediate joints" come anywhere near this degree of precision, and the jointing of the suspensions is thus recognised as the chief mechanical defect in existing balances.

My 200-grm. balance was greatly improved and yet simplified by combining the "cross joint" and the "intermediate joint" into one. A rigid stirrup was attached to the original holder of the agate plane and fitted with a conical and glass-hard steelpoint on the horizontal part from which the pan was suspended by a polished concave steel cup. The radius of curvature of a hard steelpoint capable of carrying more than 100 grm. is, however, at least 0·1 mm., and assuming a sliding angle of friction of about 6°, the exact point of application of the load remains in doubt to the extent of about 0·01 mm. in any azimuth, which is twice the calculated tolerance in the direction of the knife-edge, and corresponds, with the steelpoint at about 40 mm. below the knife-edge, to a possible tilt of the plane by $0{\cdot}01/40 = 0{\cdot}00025$ radian, or five times the calculated limit of 0·00005. Nevertheless these modified suspensions proved vastly superior to the far more complicated original ones and rendered possible the excellent results with heavy loads secured at the N.P.L. A few discordant results were, however, almost certainly due to a specially eccentric position of the steel cups causing the maximum dislocation of the point of application of the load and so greatly exceeding the permissible amount.

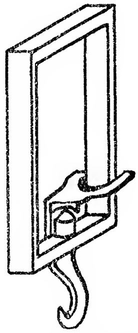

FIG. 3.—Single point intermediate joint.

A further refinement of the same idea was applied to the kilogramme balance by using a steel point of the same type on the stirrup, but providing a separate arrestment, which lifts the steel bearing from which the pan is suspended, and so allows of making this bearing practically plane without risk of accidental derangement. This appears to be the best and most complete solution of the suspension problem.

The Arrestment.

The essential features of the arrestment are that the suspensions and the beam should be gently and steadily lifted off the terminal knife-edges and the central bearing respectively, and that they should be replaced in exactly the same relative positions as before when the arrestment is again released. Freedom of the arresting bar to make slight movements in the horizontal plane is particularly objectionable, because the suspensions will then be placed upon the knife-edges in various positions in different partial weighings, and the point of application of the terminal loads will shift accordingly and cause a change of length of the lever-arm. The Mendeléeff type of arrestment, with arms swinging round an axis in line with the central knife-edge, appears to be the best, and the form given to it by Sartorius gives very satisfactory results, which, however, would be rendered still more definite if the compass joint at the centre were replaced by a strictly geometrical bearing such as is used by Sartorius at the supporting points of beam and suspensions.

An extremely trying defect of all arrestments is the adhesion of one or other of the supporting points to its counterpart, which causes one end of the beam to be pulled down a certain distance and then released with a jerk. Good results cannot be obtained when the arrestment develops this defect, and the balance must be taken to pieces and all the contact points cleaned (suitably shaped sticks of dry elder pith are best for the purpose) before proceeding. This had to be done quite frequently in the course of my experiments, but towards their end I devised a simple addition to the arrestment which is an absolute preventive of this trouble, namely, a very gentle "brake" applied to the pointer which is only lifted off at the end of the releasing movement of the arrestment. The "brake" consists of a light bent wire hanging from a supporting edge attached to the pillar, one end of the wire having a projecting end which is depressed by the arrestment when in its lowest position. The horizontal part of this wire bears with a force of about a centigramme on the pointer and keeps the balance steady until the beam and suspensions are quite out of contact with the supporting points. The result is that, even when the latter are slightly adhesive, the balance starts its

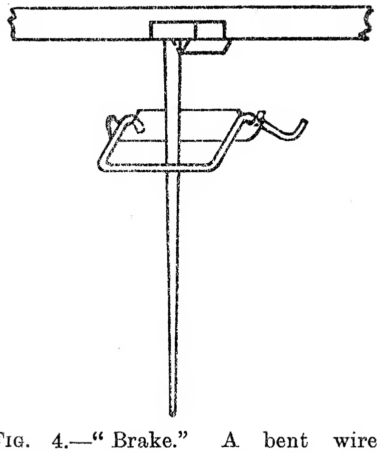

FIG. 4.—"Brake." A bent wire resting on the pointer keeps the balance steady until arrestment is completely released.

oscillations from its actual position of rest, and that a perfectly definite arc of vibration becomes associated with any given difference of the two loads. This is essential for the complete elimination of errors arising from the irregular curvature and imperfect elasticity of the knife-edges.

An extremely serious result of the action of the arrestment is that the knife-edges and their bearings come together, always in precisely the same relative position, with a finite velocity and, therefore, with a certain sudden, though minute, impact. The consequence is that the knife-edges gradually develop a distinct flat facet at the corresponding contact lines—utterly invisible, even under the highest powers of the microscope, but easily demonstrated by observation of the balance near the critical central part of its scale. When carefully adjusted and gently released, a balance will remain still in the position in which the arrestment leaves it, and will refuse to leave that position with an overweight in one pan which, in much-used analytical balances, may reach and even exceed a whole milligramme. In my small Sartorius balance, which had had very little and decidedly careful use, the necessary overweight was recently found to amount to 0·02 mgrm., which at any other part of the scale would cause a change of pointer reading by about a whole scale division. The corresponding width of the flat facet of the central knife-edge (which is the chief offender) comes out at about 1/60,000th of a millimetre, and is therefore far beyond the limit of microscopical resolving power.

Although the existence and magnitude of this grave source of error were only discovered after the completion of my experimental weighings, I fortunately escaped its effects because I reached the empirical conclusion in the earliest stages that good results were only obtainable if the time-honoured method of letting the balance swing to both sides of the central position in any one observation were abandoned, and if, instead, an ample overweight on one side or the other were permanently applied, so as to secure elongations entirely to either the right or the left side of the centre of the scale. Owing to the very pronounced damping effect resulting from the high sensitivity employed, coupled with the careful avoidance of any initial impulse, all my observations were thus made on those parts of the knife-edges which lie outside the flat facet and which consequently have a reasonably uniform curvature. It seems advisable to fit the finest balances with an auxiliary device, on similar principles to that of my " brake," which, before the lifting of the brake, would push the beam into a position slightly tilted towards the side of the scale on which the readings are to be taken, thus making absolutely sure of the avoidance of this source of error.

The Rider and its Scale.

The method of the alternating position of rest which, according to my experiments, must be employed to secure the highest possible precision in mass determinations, requires very close adjustment of the equilibrium. This is only attainable by a light rider upon a finely subdivided scale. It is a remarkable fact that it has for some considerable time been accepted as an axiom by the great Continental metrological institutions that riders are inadmissible for their balances. As an inevitable consequence, the latter do not admit of eliminating the errors due to irregular curvature and imperfect elasticity of the knife-edges, and therefore cannot reach the highest precision. The objection to the rider is that its weight-value is not nearly so definite as that of a weight in one of the pans. But I found by direct experiment with heavy riders that my 200-grm. balance had a probable error of the rider position of only 0·0001 of the rider-weight; therefore, with the really used rider weighing 1 mgrm., the probable error from this source was only 0·0001 mgrm. for one partial weighing, and quite unimportant when compared with the probable error of one complete weighing of not less than 0·0003 mgrm. It is, however, necessary to state that accuracy to 0·0001 of the rider-weight will only be obtained if the rider is placed squarely upon a scale divided by sharp and clean lines, so that the rider actually rests in the V-shaped notches of the scale-divisions. A lens magnifying three or four times should be attached to the rider-shifter, so that the exact position of the rider can be sharply observed.

General Requirements.

The case in which the balance is enclosed should, of course, be draught-proof and would probably be greatly improved if it consisted almost entirely—including the foundation-plate—of thick and well-polished aluminium, so as to equalise the internal temperature. A separate inner case surrounding the beam is highly desirable. To avoid errors from radiation pressure, the illumination should be as restricted and as symmetrical as possible. The deflections are undoubtedly best determined by the reflecting method. All my principal experiments were made by direct pointer readings, the sensitivity being 12·5 divisions of approximately 1 mm. to a milligramme. One-fiftieth of a division was estimated and the good results obtained were due to my possessing the knack of mentally subdividing a scale to perhaps an unusual degree, my probable accidental error being decidedly less than 0·01 of a division. I was also assisted by the fact that the method of alternating deflection very largely eliminates the well-known systematic error of "personal scale," for example, the habit of always reading 0·56 when a micrometer microscope would give 0·54.

The Autostatic State of a Balance.

When I first realised that I had reached an apparently unprecedented degree of precision in weighings, I felt compelled to examine minutely the whole method of observation and reduction employed, because I was myself highly sceptical as to the reality of the advance. Every detail bore the tests which I applied, with the exception of one which perturbed me for a long time: It was easily concluded, on the ordinary principles of statics, that if the level of the balance-case were to vary in the course of a set of partial weighings by only a few seconds of arc, this tilt alone would produce the whole of the probable error actually arrived at as including *all* sources of error. The balance was supported on the strongest side of the outer packing case in which it had originally arrived, the case being firmly screwed to a wall of the observing room. It seemed quite unthinkable that the table so formed should be firm within the minute limit called for. Eventually the solution of this mystery was found, and was subsequently deliberately applied, in order to secure extremely high accuracy with the 10-grm. balance when used on an ordinary dining table:—

In a sensitive short-beam balance, the centre of gravity of the moving parts lies only a very small fraction of a millimetre below the axis around which the balance oscillates. If the central knife-edge were absolutely sharp, the beam would, nevertheless, take a definite orientation with reference to the true horizon, no matter how the balance case were tilted, and the pointer would therefore correctly indicate this tilt, and the weighings would be correspondingly falsified. The central knife-edge, however, is really a cylinder of about 0·01 mm. radius, and the beam swings round the axis, C (fig. 5), of this cylinder. The balance will be in stable equilibrium if the centre of gravity, G, of the moving parts is below the axis, C, of rotation, and there are three classes of cases. If G is also below the supporting plane (fig. 5*a*), then a tilt of the whole balance, say

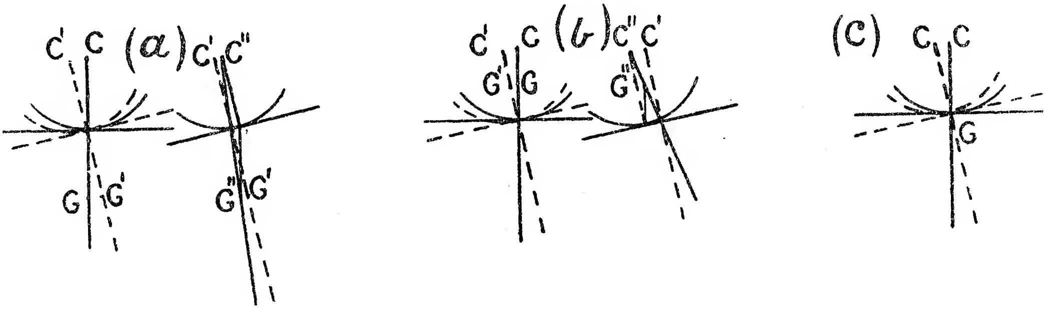

Fig. 5.—Effects of tilting the balance case.

towards the left, will shift the centre of gravity to a point to the right of a vertical plane, passing through the contact-line, and the balance will swing towards its previous orientation with reference to the horizon, but will not reach it, because the rolling of the edge on the supporting plane will bring the contact-line vertically over G before that orientation is reached. The balance, therefore, is less sensitive to tiltings of its case than one playing on an absolutely sharp knife-edge. If G is above the supporting plane, then a tilt of the case towards the left will also carry G to the left of the vertical plane of the contact-line, and the beam will swing in the direction of the tilt—exactly contrary to what one would expect—fig. 5*b*. Finally, if G lies in the contact-line, then tiltings of the case will not affect the pointer-readings at all, because the movable mass remains concentrated on the contact-line, and produces no turning moment. I propose to call this remarkable special case of balance-adjustment the "autostatic state," for a balance so adjusted gives pointer-readings which are unchanged by tiltings of the case, and are therefore independent of the *direction* of the force of gravity.

The explanation of the high accuracy obtained on the infirm support of my balance was found to be that the autostatic state was very nearly realised at the sensitivity employed in the experiments. This was not purely luck, for the sensitivity had been fixed at the selected value because it gave the most consistent results, but without any knowledge of the reason why it should do this.

As the distance CG can be easily calculated from the mass of the moving parts and the sensitivity, the experimental production of the autostatic state is the easiest method of determining the radius of curvature of the central knife-edge. The experiment consists simply in taking the position of rest, then tilting the case by shortening say, the left, and lengthening the right footscrew, and noting whether the pointer moves in the direction which simple statics would lead one to expect, or in the opposite direction. In the first case the sensitivity must be increased by the gravity-bob,. in the second diminished. The autostatic adjustment is thus quickly found. For balances on any but the firmest foundation, the autostatic adjustment is a highly desirable safeguard. It is applicable to balances read by microscopes or by reflection, provided that both scale and reading device are attached to the balance case. Balances with separately mounted scales and reading telescopes or lightspots are always placed on such firm foundations that the autostatic adjustment would not lead to any sensible gain in precision.